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A DISCUSSION ON "THE FUTURE OF GEOMETRICAL OPTICS"

Held on 21st May, 1921

A MEETING of the Society was held at Cambridge in the Cavendish Laboratory through the kindness of Professor Sir E. Rutherford who took the chair and welcomed the members of the Society. A discussion took place on "The Future of Geometrical Optics" in which numerous speakers took part. Most of the speakers subsequently made written communications which are given below.

PROFESSOR SIR E. RUTHERFORD: It is a great and very unexpected honour to be asked to preside at the first meeting of the Optical Society in Cambridge. I am afraid, however, that my own knowledge of technical optics is probably less than that of any one in the room. It is a matter of interest to remark that the meeting is held in the lecture room designed and used by Maxwell, whose interest in optical studies is well known to all. I understand that one of the objects of the meeting in Cambridge is to interest the mathematicians in the many problems of technical optics. In order to accomplish anything in this direction it seems of first importance that the mathematicians should be provided with some statement of the directions in which further advance is possible by the application of mathematical methods. Unless this is done it is difficult to see how the mathematician can know the direction in which his special knowledge will be useful. I hope that as a result of the meeting such information will be forthcoming.

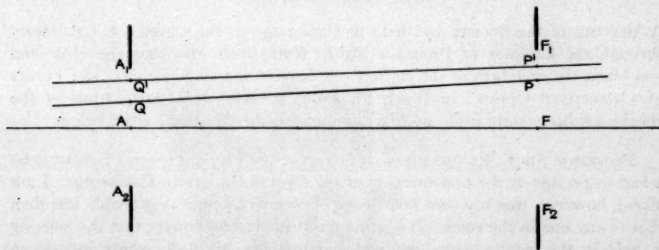
INSTRUCTOR-COMMANDER T. Y. BAKER: To the designer of optical instruments the subject of geometrical optics is of paramount importance. It is by the aid of this science that the designs of optical parts are elaborated; the necessary positions of lenses, their focal lengths, their sizes, the curves of their surfaces are obtained and calculations made as a result of which a pretty exact forecast can be made of the size, curvature, and definition of the field. The whole of the science depends almost entirely on a single physical fact that the velocity of light of a particular wave length in a particular medium is a constant for that wave length and medium,

the ratio of this velocity to the velocity of light *in vacuo* being the inverse of the refractive index. All the rest of the science, except for consideration of diffraction effects, is mathematical elaboration and the important thing for the optical designer is that the mathematical machinery should be easy of manipulation and accurate to certain tolerances depending upon the limitations of human vision.

The subject divides itself naturally into two parts: preliminary Gaussian design, and consideration of aberration. As regards the former there is but little to be said. Everything is included in the two formulae

$$1/v = 1/u + 1/f \text{ and linear magnification} = v/u.$$

In a very large number of cases, particularly of new instruments, the Gaussian design is completed first. The requirements of the instrument are known; there must be a field of such and such size, there must be an exit pupil of certain dimensions, there must be certain places in the instrument where graticules could be placed, and so forth. All such work is adequately treated in most text books



although perhaps in some, particularly in English, the importance of the determination of the sizes of the lenses required to let through the requisite amount of light is not always sufficiently emphasised. It is also desirable that calculation formulae should be put into the most practicable form. The Gaussian design being thus determined, there remain still available for the designer the *shapes* of the different lenses. For any simple lens with air on both sides the shape can be varied at will from an extreme meniscus which is hollow to the entrant light to an extreme in the opposite direction. For every simple lens there is therefore a parameter available for the purpose of elimination of aberrations and in general these parameters can be and are so used.

What then can mathematics do for the designer to enable him to calculate aberration in general terms involving the shapes of the lenses so that when these general terms are written down and equated to zero he may treat them as equations from which the lens shapes may be solved? So far I am afraid very little of real practical use has been done. Consider for a moment the method of image formation in one of the spaces separating two lenses in an instrument. There is in that space by Gaussian computation an aperture plane and stop A_1A_2 (see Fig.) and a

field plane and stop F_1F_2 . From the axial symmetry of the instrument—and nearly all optical instruments are axially symmetrical, at all events optically—each of these stops is circular in shape and of a radius that is easily calculable. The size of the aperture stop depends on the required size of the exit pupil, the size of the field stop upon the requisite size of the final field of view.

From the Gaussian calculation the formation of a point image at P in the field plane will be made by a whole series of rays which pass through A_1A_2 . But Gaussian optics, which assumes a point to point correspondence between object and image spaces, is only approximately true, the degree of approximation being taken to be that the inclinations with the axis of all rays which form the images are so small that higher powers than the first of their circular measures are negligible. Now from the practical standpoint such is far from being the case. For instance in an instrument used for visual purposes a field of view at the eye of 55° is often needed. Rays from the edge of the field therefore need to be inclined at an angle whose circular measure is one-half and that value must be raised to more than the twelfth power before it becomes smaller than the one minute which may be taken as representing roughly the limitation of the human vision.

To return to the figure, let us consider the Gaussian ray QP crossing the aperture at Q and the field at P . If we work back by Gaussian formulae this ray corresponds to a certain ray in the initial space, and if we now retrace this ray by exact trigonometrical formulae it will be found that in the particular space under consideration it neither crosses the image plane at P , but at P' , nor the aperture at Q , but at Q' . The quantity PP' represents a defect in the image formation and one of the functions of the science of geometrical optics is to express PP' in terms of the parameters of the system.

The most obvious way to do so is in a power series. If α represents on the field stop the height of the Gaussian point and β the height on the aperture stop of the point Q , then PP' can be written in the form

$$A\alpha^3 + B\alpha^2\beta + C\alpha\beta^2 + D\beta^3 + 5\text{th degree terms} + 7\text{th degree terms} + \text{etc.}$$

The cubic terms represent what the optician calls the first order aberrations; the fifth power those of the second order, and so on. Actually the statement is more complicated than this, for it is necessary to consider not only rays which lie in an axial plane but also those which do not cross the axis at all. For consideration here, however, the axial rays will serve sufficiently well. The coefficients A , B , C , and D are well known as expressing the defects of spherical aberration, coma, astigmatism, and distortion. They can be written down in general terms involving the image and stop positions, and the lens shapes. This has been done in many different ways and they are separately zero if four of the five Seidel conditions are fulfilled.

In text books on optics an examination of these first order aberrations is generally to be found, but in English works I think there is a great deal of room for improvement. For often this examination is treated in a piecemeal fashion. The coefficient A is examined very carefully because it leads to the development of the axial caustic. The next coefficient B is usually dismissed with a reference

to the Sine Condition. Astigmatism represented by C is developed further since it can be connected with the focal lines of thin pencils, but the distortion coefficient D is very frequently omitted altogether.

I should like to see a much greater development of the investigation of these first order coefficients. To take the simple case of a single lens used with a stop not placed at the lens, all four coefficients are quadratic functions of the lens shapes. We ought to have in a text book on geometrical optics a most thorough investigation of the range of values that the coefficients can have as the image and stop positions and the lens shape are varied within practical limits. I have seen it stated, for example, that A can be made to change sign and a simple lens obtained which is over-corrected for spherical aberration. In a sense this is true but the amount of over-correction is minute and is only obtainable by the use of lens forms that are hopelessly impracticable.

In a certain sense then it may be said that algebraic formulae are available by means of which first order aberrations can be written down for a system of lenses, and to a certain extent optical designers make use of such formulae. But in making that statement it is not to be assumed that an instrument can be wholly designed from an elimination of the first order coefficients. The actual aberration consists not only of these first order terms but also of the fifth power terms, of the seventh power and so forth; and these higher order aberrations are by no means negligible when we deal with modern instruments with wide apertures and large fields. If the designer attempts to use general expressions for aberrations of higher order than the first he quickly finds himself in hopeless difficulties. The forms of the coefficients of the second order are far longer and more complicated than those of the first and become much too unwieldy to use. So much is this the case that nearly always first order aberrations are used for the purpose of obtaining an idea of the direction in which a solution of the problem is to be obtained, and having done so the designer proceeds to calculate by trigonometry the displacement between P and P' and to modify his curves so as to make that displacement reasonably small all over the field. It is to my mind at this point that new mathematical investigation of the aberrations is needed. Trigonometrical methods are long and they give no indication whatever of the direction in which variation of the parameters is to be made in order to reduce the typical PP' to a negligible amount. Rapidity of design is possible only by a very complete experience of what kind of change will be produced by making certain alterations in the curves, and until the designer has been years at the work his intuition in this respect will be very unreliable.

If we are to make advances in calculation methods it becomes imperative, I think, to replace the power series by something else. For one reason the power series is not always convergent, and unless it is convergent it does not represent the aberration at all. I may perhaps be pardoned if, for the benefit of non-mathematical readers, I explain by means of a simple example what is meant by that statement. A particular case of the binomial theorem enables us to write

$$1/(1-x) = 1 + x + x^2 + x^3 + \dots \text{ to infinity}$$

and we can verify that this is true by substituting numerical values for x . If we put $x = 1/2$ we obtain for the value of the series

$$1 + 1/2 + 1/4 + 1/8 + 1/16 + \dots$$

and it is quite obvious that the more and more terms are included the closer does the sum approach 2 and that in the limit when we take in an infinite number of terms, the sum is exactly 2, which is the value given when we substitute $x = 1/2$ in the expression $1/(1-x)$. If we put $x = 1/10$ the series becomes

$$1 + 0.1 + 0.01 + 0.001 + \dots = 1.1111 \dots = 1\frac{1}{9}$$

and again the equivalence between the series and the fraction is verified. But if we put $x = 2$ there is an obvious breakdown in the formula. Clearly we cannot say that

$$1 + 2 + 4 + 8 + 16 + \dots$$

is equal to -1 .

In mathematical language the radius of convergence for the series is 1. As long as we limit the use of the series to values of x which are not greater than 1 it is fair to say that by taking a sufficient number of terms we can make the sum of the terms of the series approach as close as we please to the value of the fraction. To get any required degree of approximation the number of terms required will depend upon the value of x . For instance to get a 99 per cent. accuracy in the case of $x = 1/2$ we must include six terms, but in the case of $x = 1/10$ only three terms are required. Thus the rapidity of convergence depends also on the value of the variable. In general it is evident that a value near to the radius will give slow convergence.

If then the power series is to be used to express the aberration it must be convergent for all values of α and β that the Gaussian design enjoins and also if it is to be practically useful it must be so rapidly convergent that at the most second or third order terms represent the limit to which the terms of the series need be considered. These conditions are hardly ever fulfilled. I should estimate, roughly, that in one instrument out of ten there will be some place where the convergency breaks down completely for rays which go to the formation of the image as actually seen, and that in five the convergence is so slow at some place that at least fifth order aberrations would have to be considered, and if fifth order aberrations have to be included at one place they must be included at all places.

For these reasons I state emphatically that the expression of aberrations in power series can never be a practical method by means of which instruments can be designed. But there appears to me to be no reason why it should not be possible to evolve other expressions approximately equal to the aberrations that can be made to serve the purpose of general design and it is in such direction that the future development of the subject appears to lie.

Let me illustrate the point still further by a small development of the example of convergency that has been considered above. We know that if the power series is convergent

$$1/(1-x) = 1 + x + x^2 + x^3 + \dots$$

and that approximations to the series could be made by taking first two terms $1 + x$, or again three terms $1 + x + x^2$ or more terms. If we tabulate these results we have

	$x = 0.1$	$x = 0.2$	$x = 0.3$	$x = 0.4$	$x = 0.5$
$1/(1-x)$	1.111	1.250	1.429	1.667	2.000
$1+x$	1.100	1.200	1.300	1.400	1.500
$1+x+x^2$	1.110	1.240	1.390	1.560	1.750

Clearly we get a closer approximation to the truth by taking the longer expression $1 + x + x^2$ than by taking the shorter one, but we do get a considerable error as x increases up to 0.5.

If instead of taking terms of the series we endeavour to find other functions of x which are approximately equal to $1/(1-x)$ we may perhaps hit upon one which is closer to the true value over the range required than either $1+x$ or $1+x+x^2$.

For example try $\frac{1}{(1-\frac{1}{2}x)^2}$. The tabulated values are

1.107	1.234	1.384	1.562	1.779
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The values are in all cases better than $1+x$ and in two cases are better than $1+x+x^2$ but not so close for the other three. Or again, try

$$\frac{0.355x}{10^{1-x}}$$

and we find for the values

1.095	1.227	1.419	1.724	2.265
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Obviously an infinity of different forms can be found which represent approximately the same values as these of $1/(1-x)$ over the range $x = 0$ to $x = 0.5$.

Just as it is possible in this simple case to replace the power series as an approximation by other forms of functions so I am sure it is possible in the case of optical aberrations, and I am of the opinion that a mathematician's time would be better spent in endeavouring to build up such approximate forms than in finding the general coefficients in the higher order terms of the power series.

What the form of such a function should be in the general case of an aberration off the axis I am chary of suggesting. It must be a function of

- (1) the distance of the image point P from the axis,
- (2) the distance of the aperture point Q from the axis,
- (3) the angle between the axial planes through P and Q ,
- (4) the linear magnification of each image as formed after refraction through successive lenses,
- (5) the successive lens shapes.

The form chosen must be such that the value given for the aberration must be in close accord with the truth for all reasonable variations in every one of the five variables mentioned above. The form must be such that it allows of the calculation of parameters dependent upon (4) and (5), which can be easily carried out or can be tabulated for reference. It must be such that the parameters used for one

system can be compounded with those of another, and the parameters of the combined system thus obtained. If such approximate formulae for aberrations can be found then we shall be able to write down in general terms the final aberrational parameters for a given system and by equating them to zero, say, solve equations for lens shapes.

That such a scheme is possible I feel convinced from the results obtained by Professor Filon and myself in regard to aberration on the axis. In this case we had a simpler problem, it is true, but it illustrated in a very nice manner the possibilities that exist in using functions other than power series. We dealt only with longitudinal spherical aberration, which put into a power series would be of the form

$$At^2 + bt^4 + ct^6 + \dots$$

where t is the tangent of the inclination of the Gaussian ray to the axis and A, b, c are parameters involving the image position and the lens shape. The stop position does not come into this investigation at all except in so far as it limits the value to which t can rise.

We found that the first order aberration At^2 was very frequently far from being a close enough approximation; we found that for some image positions, notably in the neighbourhood of the front focus, the series was divergent; we found that for a very wide range of image position and of lens shapes first order aberrations were nowhere near the true values; we found that it was necessary in cases where the series was convergent to go in many cases far beyond the first order terms in order to obtain expressions that could be looked upon as reasonably close to the true aberration.

We then tried to find empirically a formula which would express the longitudinal aberration in fractional form instead of as a power series and found that if the longitudinal aberration were written

$$\frac{At^2}{1 + Bt^2},$$

A and B were functions that could be calculated from a knowledge of the lens shape and the image position*. We found that to get a closer degree of accuracy it was advisable to modify the formula to

$$\frac{At^2 + Et^4}{1 + Bt^2},$$

and when this was done the mean percentage error for a wide range of image positions and a wide range of lens shapes was something like one part in 500. Further we showed that after crossing a lens the direction of the emergent ray, q , in terms of the direction of the emergent Gaussian ray, could be also expressed approximately in the form

$$q = t(1 + Bt^2)/(1 + Ct^2).$$

We showed also the functional form of the coefficients A, B, C , and E . The first three are of the second degree in lens shape, and of fourth, third, and second degree respectively in linear magnification. The quantity E is of fourth degree in lens

* *Trans. Opt. Soc.* 20 (1918), 67; *Phil. Trans. Roy. Soc. Series A*, vol. 221, p. 29.

shape and sixth degree in magnification. Further we showed how the constants of one system could be combined with the constants of another to produce the constants for the whole.

I think therefore that, in arguing that a serious attempt should be made by mathematicians to evolve new forms for expressing aberrations, Professor Filon and I can point to the achievement of a distinct degree of success in this respect as far as concerns aberration at the centre of the field. It is quite probable that there are other forms that would do as well; forms which will give a closer degree of approximation to the truth for all reasonable cases that may arise; forms which contain the constants which depend upon lens shape and image position in a simpler manner; forms which are more easily adapted for the composition of one system with another.

To make a thorough exploration of the whole subject demands more than one man's lifetime, but I feel confident that there are such hopeful possibilities in what I have sketched out that in a few years' time no one will use trigonometrical methods for instrument design at all.

PROFESSOR L. N. G. FILON: In discussing the future of geometrical optics it has to be remembered that a subject of this kind can flourish only if there is interchange of thought and active cooperation between the mathematician on the one hand and the practical designer and manufacturer on the other. If such cooperation is absent, then the outlook for this branch of knowledge is a poor one. As to the methods of obtaining it I would merely say that improvement is not to be looked for in an attempt to turn out mathematicians who are also practical designers, or to expect business opticians to acquire the mathematical knowledge necessary to advance the mathematical parts of the subject. Such an attempt is bound to fail; the two types of mind are distinct. To try and blend them will produce only a sterile hybrid. What is wanted is effective and intimate partnership between individuals, each partner being an expert in his own line, but familiar with the *language* of the other.

At present geometrical optics, considered as a mathematical subject, is suffering from a setback. The mathematician has lost interest in it and I have heard it described, in mathematical circles, as a "dead" subject. I rather agree with Commander Baker that this frame of mind is probably largely due to the manner in which the subject is treated in the ordinary English text book. For one thing, the treatment of aberrations is often very scrappy in character, as pointed out by the opener of the discussion. There is no real reason why this should be so. It is well known that the four aberrations of the first order (a mathematician would prefer to refer to them as of the *third* order) are derivable by quite simple processes of differentiation from the aberration coefficient A , expressed as a quartic in the magnification in the form

$$A = A_0 + A_1M + A_2M^2 + A_3M^3 + A_4M^4.$$

So far as this order of aberrations is concerned, the five coefficients $A_0, A_1, A_2,$

A_3, A_4 are sufficient to define the characteristics of image defects in the system completely, and it would therefore appear natural to base the study of aberrations upon this quartic, as has been done by Mr T. Smith.

Further, the ordinary text books do not seem to pay sufficient attention to the combination of systems and the deduction of the coefficients for the compound system. It is readily shown that any coefficient A_n for the compound system is expressible as a linear combination of the A_n 's of the components, the coefficients being functions of the focal lengths, which, in any problem of design, are usually given beforehand. The A_n 's for a lens are quadratic functions of a "shape" coefficient, K , of the lens; K being in fact the product of the focal length by the mean curvature of the two surfaces, a quantity frequently at our disposal. The formulae of combination then exhibit at once the effect upon the final result of varying the shape of any individual lens, and this, I believe, is the kind of result wanted by the scientific designer.

It is, of course, well known that first order aberrations are usually inadequate and, in fact, in many cases, are not even an approximation to the true value. This raises the whole subject of the method of successive approximations by means of series, and whether such series are convergent. Generally these series will proceed by powers of some quantity x , which is either the inclination to the axis of the incident or emergent ray, or the sine or tangent of such inclinations. It is a fundamental result in algebra that if the value of x numerically exceeds a certain value ξ , which the mathematician calls the radius of convergence, then the terms of the series increase without limit in absolute magnitude and the series is then arithmetically meaningless. It is therefore essential that such series, when employed in optical calculations, should have a radius of convergence not less than the extreme value of the argument x corresponding to rays actually passing through the system.

Now it can be shown* that in actual optical examples this is very frequently not the case, and that the radius of convergence of the series for the optical aberrations depends very greatly both upon the choice of the argument x and upon the position of the axial crossing-point of the ray dealt with. If x is the emergent inclination, or its sine, or its tangent, then, whenever we are dealing with an emergent ray nearly parallel to the axis, but crossing the image plane at a finite distance from the axis, it will be found that the radius of convergence of the series approaches zero, so that the method of successive aberrations is then useless in the majority of cases.

Such rays are bound to occur in almost every emergent pencil, so that, as soon as we consider aberrations off the axis, grave inaccuracy is bound to be introduced by the method of successive aberrations if the emergent inclination is used as argument. If, on the other hand, we develop in terms of the inclination of the incident ray, the difficulties of convergence are not so great, that is to say, there are some forms of aberration series which, in general, always have a finite radius

* T. Y. Baker and L. N. G. Filon, "On a theory of the second order longitudinal spherical aberration in a symmetrical optical system," *Phil. Trans. Roy. Soc. A*, vol. 221, pp. 29-71.

of convergence, but others, in particular the longitudinal spherical aberration, have not. We have here an important line of research, involving more advanced theory of functions, which should make an appeal to the pure mathematician.

Further, it seems doubtful whether, in dealing with aberrations, it is wise to follow on the lines of developments in series, and whether series are really the natural form belonging to this subject. I am the more inclined to doubt this that recently Commander Baker and myself have succeeded in throwing the second order longitudinal aberrations into a fractional form

$$\frac{At^2 + Et^4}{1 + Bt^2},$$

t being the tangent of the inclination of the Gaussian emergent ray, and A, E, B being polynomials in the magnification of degrees 4, 6, 3, and this fractional form gives a better fit than any other known second order approximation; and further it so happens that, when the aberration is expressed in this form, the A, E, B of a compound system are expressed in terms of the A, E, B of the components in a peculiarly simple manner, the result being linear in the coefficients belonging to each lens separately.

At present, thanks to the assistance of the Department of Scientific and Industrial Research and of Professor J. W. Nicholson, F.R.S., tables of these functions are being calculated and it is hoped that, eventually, optical designers may learn to use them.

Another consideration which has militated against proper attention being given to this subject by the professional mathematician is that, so far, it has been merely heavy algebra and has afforded little scope for the use of the methods of the differential and integral calculus. Hence, from the point of view of the senior student desiring to acquire familiarity with advanced mathematical methods, its educational value has been slight. Here again it may be that workers have not directed their attention into the right channels. Differential relations of a simple kind do undoubtedly exist between various aberration coefficients. Thus, if we denote by Q the coefficient of first order aberration in the sine ratio, so that

$$\frac{\sin \alpha_2}{\sin \alpha_0} = \frac{1}{M} \left(1 + \frac{Q \sin^2 \alpha_0}{M^2} \right),$$

α_0, α_2 being the incident and emergent inclinations and M the angular magnification, then it can be shown that

$$Q = \frac{1}{4} \frac{dA}{dM} - \frac{1}{8} (1 - M^2)$$

is an invariant differential relation holding good for any optical system. The real geometrical significance of such a relation needs, however, to be brought out, and it might lead to the discovery of new and important invariant differential relations which would invest the whole subject, in the eyes of the mathematician, with much greater interest and elegance and might lead to a notable simplification in calculations.

It seems rather lamentable that a laborious method of trial and error, by tracing trigonometrically the paths of a few selected rays, is the best that our designers can do; a state of things which could hardly exist, if mathematicians realised how much, and in which directions, their assistance is required.

MR HORACE C. BECK: As Commander Baker has pointed out in his very interesting remarks, the present method of calculating lens aberrations is extremely unsatisfactory. When lenses are considered without their aberrations in the manner generally called the first approximation, the Gauss theory gives an excellent method of treating the subject and far more can be learnt about optical systems in this manner than is usually realised. After this there are the methods Commander Baker has referred to, called the second approximation, in which the second term in the series of the expansion of the sine is taken. Such methods are applicable to some telescope objectives and other lenses where only small angles of aperture and of view are used. From these equations certain conditions have been laid down but these are not satisfactory and in many cases are positively misleading. In most optical systems the corrections are a matter of compromise and, if a small amount of aberration of one sort is left in, the formula necessary for its complete deletion may be entirely modified. As an example, take the Petzval condition. To get a telescope object glass to give the best covering power over 3 or 4 degrees, this probably must be fulfilled, but in making a photographic lens some years ago, when a large angle of field was required in which the covering power was of the very highest order, I found that a lens which gave a perfectly satisfactory image at 35 degrees from the axis, entirely free from astigmatism, should according to the Petzval condition have had at least $2/3$ of the aberration of an ordinary rectilinear lens. As a matter of fact, a critical investigation of the field of this lens showed a slight curvature at between 5 and 10 degrees from the axis but the confusion due to this was not so large as the grain on a photographic plate.

We should, therefore, very much appreciate a system which would enable us to solve optical systems direct but I do not think it is probable we shall get one as the conditions are so complicated. I am at the present time working on some apochromatic objectives in which there are between thirty and forty variables, and an enormous number of aberrations to correct. These aberrations are a good deal more complicated than many people realise. Achromatism, for instance, is usually considered a very simple matter but the enormous effect of thickness is not always realised. In an ordinary plano-convex hemispherical lens of crown glass the equivalent focus is of course the same on both sides of the lens but the distance on the axis between the focal points for *C* and *G* on one side of the lens is only approximately 88 per cent. of what it is on the other. This is caused by the alteration in the position of the principal planes, but as these move as a function of μ and the alteration of the equivalent focus as a function of $\mu - 1$, this alteration alters the partial dispersion of the lens, a very important matter in apochromatics.

Then as regards spherical aberration, it is not always understood that it is not sufficient to make all rays which leave one conjugate point meet at the other one,

but in addition they must come at the correct angle. Again, rays going through different zones of a lens, which is not perfectly corrected, come to the axis either in the wrong position or at the wrong angle.

The present method of trigonometrical calculation is nothing more than the old fashioned rule of thumb method of trial and error. In lenses where it is easy to measure the aberrations accurately, it is much more rapid and cheaper and quite as satisfactory to measure the results and by tabulating and examining the aberrations, methods of eliminating them can be found.

In an apochromatic objective I recently calculated I made up a model when from the calculations it was approaching the corrections required. When I saw it, I made some alterations to it which entirely corrected it and then on calculating over the results so obtained, it showed that the greatest difference in the equivalent focus for different zones was under $\cdot 1$ per cent. and the greatest error in angle $\cdot 03$ per cent.

Every alteration to a lens alters most of the corrections and some other alteration has to be introduced to counterbalance it. Now if the mathematicians would show us some method by which we could say which two or three variables should be altered to affect one particular zone without affecting the others, it would be of great assistance to us as it might very largely reduce the tracing of rays through surfaces which in some of the calculations on the present methods may run into many hundreds and even thousands.

PROFESSOR CHESHIRE: We are met here to-day mainly, I take it, for the purpose of interesting Cambridge mathematicians in those problems which concern us more particularly as opticians, and in particular the problem of the designing and computing of optical systems. It is therefore of considerable importance that in presenting this problem we should make it perfectly clear at what stage we have arrived. As to this I think there can be little doubt. Leaving out of account the work of such optical geniuses as Mr H. Dennis Taylor, it can be fairly said that first-class modern work, almost without exception, is done by purely trigonometrical methods, and so completely that the optical properties of a computed system can be determined more accurately upon paper than by any possible workshop experiments made with actual systems.

In about the year 1903 I had the privilege of spending some time in the Works of Carl Zeiss, and amongst other matters I was anxious to find out what the practice of that firm was in the matter of computing and designing. I was told that Professor Abbe had introduced many important improvements in their methods, and that the rule had been rigidly laid down that the designing and computing of optical systems must be completed in the computing office without any recourse to workshop trials. The practice of Carl Zeiss is, I believe, the practice of all first-class opticians. The workshop is not called upon until the constructional data for an optical system have been completely and satisfactorily determined. The system thus finally made in the workshop does not, therefore, test and determine the value of the design—the work of the designer—but rather the skill and accuracy with

which the workman can produce a predetermined optical system. I suggest that, as we have present to-day so many representatives of our best English firms, they should be asked individually to give us their experience upon this important matter.

MR J. W. HASSELKUS: I should like to compliment Commander Baker on the clear way in which he exposes a want that is much felt by optical designers. At the present stage of optical mathematics there are no workable formulae which would evolve the best form of lenses to be used, for example, in a photographic system of $F/4.5$ aperture. I am referring to formulae such as are quite well known for the two lens uncemented telescope objective. From the very extensive literature on the subject, it must have fascinated a good many mathematicians, and could a purely analytical method be found to treat complex optical systems like photographic objectives on similar lines, I feel sure that mathematicians all the world over would take up the matter, primarily perhaps for the joy of abstract mathematics but ultimately to the benefit of the optical designer. I do not think that to-day any designer would begin to get out formulae *de novo* for a two lens telescope system. He would work with any one of the formulae by Moser, Harting, von Hoegh and others.

To show the difference between the two systems, i.e. telescope and photographic lenses, I will enumerate the conditions that have to be fulfilled in each case.

A. The Telescope Objective.

1. Definite focus.
2. Achromatic for two colours.
3. Spherically corrected for one colour.
4. Sine condition fulfilled.

Given the two glasses and the focus required any one of the formulae referred to will give the four radii. A slight correction only is required for thickness and aperture. The limitations of these formulae are angle of view and aperture. They fail when the one is beyond 1 or 2 degrees and the other beyond $F/12$ to $F/15$. But for the usual run of telescope objectives they are infallible.

B. The Photographic Lens of $F/4.5$ aperture.

1. Definite focus.
2. Definite ratio of aperture to focus.
3. Achromatic for two colours.
4. Spherically corrected for one colour over the whole aperture.
5. Sine condition fulfilled over the whole aperture.
6. Coma corrected over the entire field and whole aperture.
7. Astigmatism corrected over the entire field.
8. Distortion corrected over the entire field.
9. The field should be corrected over an angle of 60 degrees.

These nine conditions *must* be fulfilled in any photographic lens, but, according to circumstances, a number of other conditions may have to be added.

- (a) The complete lens system should not be longer than its diameter.
- (b) There should be two symmetrical components in the system with the iris diaphragm between them.
- (c) Both components should in themselves be satisfactory photographic systems to enable them to be used separately or as combinations of various foci.
- (d) The back focus may be required to be considerably shorter than the equivalent focus (telecentric lenses).

The designer's work consists in constructing a system which fulfils within practical limits all the conditions that have been laid down. He must complete his calculations to such a degree that when a system has been made in the Works to the stated data, which comprise the indices of the various glasses, the radii, thicknesses, diameters, and separations of the lenses forming the combination, it must perform, i.e. the image given by the objective should be good, having regard to the aperture and angle of field laid down in the conditions. There should be no necessity for subsequent tinkering to improve the performance once the objective is made according to calculations.

The process of arriving at a satisfactory design of objective is in itself a method of trial and error, but trial and error on paper, not in the workshop. The effect of an alteration in any of the elements can be traced right through the system and its result on the performance determined.

The greatest difficulty confronting the designer is the uncertainty in selecting the best way to compromise the inevitable aberrations, or expressed a little differently, how far it is advisable to correct one kind of aberration to the detriment of another. Here experience is invaluable and a guide is often furnished by the special object in view when designing a system. I will mention one instance only; the compromise necessary to make a telecentric lens with short back focus perform reasonably well is totally different from that required to make an unsymmetrical photographic lens of the ordinary type of equal equivalent focus and aperture.

The work of the designer would be immeasurably lightened if analytical formulae such as I have mentioned in connection with telescope objectives were available for complex optical systems, and they would undoubtedly in the hands of a capable designer lead to entirely new forms of lens systems.

To my knowledge the only photographic lens entirely calculated by analytical methods has been the Petzval lens. Unfortunately Petzval did not publish his method, nor his formulae, but it has been stated that he carried his formulae far enough to include the ninth power. Judging by the perfect correction obtained in his lens this must have been so.

In concluding my remarks I would express my sincere hope that Commander Baker's appeal for mathematical precision tools will not go unheard.

MR CONRAD BECK: Professor Cheshire has asked what are the methods adopted in optical factories for designing optical instruments. He has been told by members

of the staff of Messrs Zeiss that they computed an instrument on paper and had it made up in the factory to the exact data thereby obtained, and that no further alteration was necessary. This was no doubt accurate, but probably the modesty of the members of the staff prevented them from pointing out that the secret of the success in these instruments was not the computing but the invention that lay behind it.

No one can make a new instrument by computation. There are no mathematical formulae capable of solution that will give the data for an optical system that may have anything from 20 to 60 variables. Computing is no more than a clumsy trial and error method of testing the design and improving it in detail.

What then is the greatest help for providing the invention that is necessary to produce new types of instruments? The invention becomes very similar to that required for other branches of science. In order to obtain fresh ideas, it is necessary to have a thoroughly good education and knowledge of the subject, combined with experience. It is then necessary to state to oneself the conditions of the problem to be solved with the utmost clearness, to consider the hopeful lines of solution, and by a process of elimination to discard the impossible or unsuitable.

During this process new ideas are likely to develop, some of which may be sufficiently promising to be worth computing. The study of the Gaussian system of optics and the examination of mathematical formulae such as those of Herschel and Coddington or others on the lines suggested by Dr Filon are of the greatest possible assistance. A mathematical formula may be of no practical use for direct application but may be of the utmost value in revealing tendencies, and pointing out unproductive directions of research, and if mathematicians can work out mathematics in such a way as to produce formulae governing the solution expressed in various forms as regards the means of approximation they will have the greatest value from an educational point of view.

The best book on the Gauss theory is a recent translation of a book by Ferraris made by Mr Oscar Faber, and published by H.M. Stationery Office.

MR F. TWYMAN: Geometrical optics may perhaps be defined as optics ignoring the wave theory, and, *a fortiori*, the electromagnetic theory. The attempts to express the later generalisations of geometrical optics in terms of the wave theory have so far only increased their already serious complexity. The unavoidable incompatibility between spherical surface refraction and an ideal optical collineation is a sufficient burden in itself without adding to it the distressing phenomenon that increased knowledge only adds to the computer's difficulty.

Commander Baker and Dr Filon have certainly shown us that this hitherto arid, unbeautiful, and laborious science of the passage of rays through spherical lenses is susceptible of yielding fresh generalities making towards simplification, and it may be that in so doing they have given the conception of a light ray a fresh lease of vigorous and valuable life. They certainly seem to promise some immediate help. I feel, however, that the main hope of deriving manageable formulae for the aberrations lies in work more directly in touch with the present state of

physical knowledge. Perhaps such a hope is not justified, but at least the aberrations, if expressed in terms of the shape of the wave front, would be in a form permitting the distribution of light in the neighbourhood of the focus to be determined—a position in which even an infinite number of ray tracings would not place us.

DR R. S. CLAY: Shortly after leaving Cambridge I had to design a simple optical system and looked up my old lecture notes to see if they would help me. I found that, however useful the formulae I had might be for the examination of a completed system, they were quite useless for the design of a new one.

It is a great pity that the beautiful work of Coddington—who was himself a Cambridge man and a Fellow of Trinity—has not been carried further. It is still the most suggestive for a first design, and is of course the foundation of Dennis Taylor's system of optics. If it were developed, many rules and formulae of great value to the designer could be obtained. The trigonometrical method, which is so relied upon by some opticians, though valuable as a final check, is of little or no use for the first groundwork, as it affords no suggestions for any radical departure from an ascertained pattern.

If a suitable linkwork were devised—somewhat on the principle of the Peaucellier cell—with the experience in accurate workmanship which has been acquired in the war it ought to be possible to obtain a first approximation to, or at any rate valuable suggestions for, the design of wide-angle optical systems. Such a linkage could probably be made to an accuracy of 2 or 3 parts in 10,000, perhaps even 1 part in 10,000.

MAJOR E. O. HENRICI: I have been asked to say something on the attitude of the Department of Scientific and Industrial Research on the question under discussion. Their interest is, I think, sufficiently shown by the following facts, most of which have been published in the annual reports of the Department. The Department was formed in 1915. In 1916 a standing Committee on Glass and Optical Instruments was started. One of the first results was the cooperation with the London County Council and the Board of Education which led to the formation of the Department of Technical Optics at the Imperial College. The Research Department have given an annual grant of £1000 towards the expenses. This grant will cease after this year, but I understand that a corresponding grant will be continued in future by the University Grants Committee of the Treasury.

The Department also early arranged for the publication of revised versions in English of the works of Gleichen and von Rohr, and both these have now been published. The former is sold out, and a second edition will shortly be issued.

The British Optical Instrument Research Association (now the Scientific Instrument Research Association) was one of the first to be started, and in view of the special circumstances of the case a very much larger proportion of the expenses of the Association is being defrayed out of public funds than is usually the case.

The Department is always open to consider personal grants to individual investigators, and considerable grants of this nature have been made for the purpose of working out methods for computing optical systems.

I do not propose to say anything about the results obtained, as there are others present who are far more competent to do so than I am, but I think I have said enough to show that the Research Department realise the importance of the subject, and have given substantial assistance.

CAPT. A. W. F. McEWAN: The general public is often told that the Germans defeated the British Optical Trade by mass production, but those of us who are members of the trade know better. They defeated the British trade with the forces of design and reputation and then, like good generals, sought by mass production to turn defeat to rout—an operation in which they were on the eve of success when the Great War intervened.

It is necessary to repeat and emphasise this the true order of historical facts, because vigorous mass production and a little ray tracing may have been good enough for the British Optical Trade under war conditions—the necessary complement to mass production was the mass destruction of war, and a little ray tracing sufficed since we in the Navy and Army had to accept whatever was given us. But industrial conditions are quite different; then the complement of mass production must be mass selling; and as for a little ray tracing! Well! a public, either at home or abroad cannot be reduced for long to accept articles if a superior source thrusts itself forward. The British Optical Trade must create in the next four or five years both original design and world wide reputation. The Germans may be counted on with absolute confidence to renew optical hostilities.

It is difficult to see how, as a commercial proposition, adopting the trigonometrical method of design alone, the British designer is to overtake and pass the Germans. In this method, organised team work, organised experience, and the careful classification and filing of years of past calculations count for so much. Those who have grown bald and grey headed at ray tracing are the first to admit its defects as a method of design and long for more powerful tools, but regard that as impossible. In their opinion experience and trial and error are the only roads to design. One must not hastily set such men down as pessimistic and unscientific: may I recall that at least one other physical problem has to be tackled in the same way—I allude to the design of the form of a ship. In my opinion ray tracing must always be the method by which the final workshop design is computed: but what I call the "philosophic form" of the design should be found by analytic methods; by "philosophic form" I mean a design which is, so to speak, 85 per cent. correct. Give that to a competent ray tracer and he will get out all that is possible of the odd 15 per cent. far more accurately and cheaply than could be done by analytical or experimental means.

The first great principle of optical design is to keep aberrations *out* of a system. The second great principle is the efficient and proper use of the glass maker's catalogue. The third great principle is to know what, and how much, is possible

for a given cost price (i.e. number of surfaces worked, etc.) or other given conditions. Analytical methods alone can make certain of proper observance of these principles.

My own researches, directed towards the creation of a *workable* analytical system which could be depended on to produce the required "philosophic form," are in the muddle I left them in 1914. But accepting them in that stage of development I have recently, by the kindness of Mr Hasselkus, been given the chance to try them against a difficult problem and get the results criticised and compared against the large library of calculations made and recorded in the mathematical offices of Messrs Ross, Ltd. My conclusions on the whole subject are broadly as follow:

1. The commonly used angular and linear methods of describing aberrations are, for analytical purposes, undesirable; I was very surprised at this result and accepted it with reluctance. All the classical work of the past has been done in one or other of these ways.

2. I was driven, at first against my will, to employ a time method of measuring aberration. Once the method was accepted and developed I noticed that only five aberrations were needed to express the phenomena to any desired degree of accuracy: for instance, with an angle of incidence of about 30° on an air-glass surface the aberration (*not* the power or focal length) could be approximated to 3 or 4 parts in a hundred thousand or a million; I forget which. This was far in excess of my requirements and I did not develop this approximation.

3. In passing from surface to surface, laws of symmetry and behaviour were found to exist. These were under consideration when I left the work in 1914: also the question of the change in the nature of an aberration as the focus of a pencil is approached.

4. The result of the experiment made by courtesy of Mr Hasselkus showed me that whilst qualitatively my results were sound, quantitatively they were a complete failure, due to the incomplete and unsatisfactory work mentioned in paragraph 3.

I believe that a useful analytical system is not an impossibility. It may be accepted that only five aberrations need be considered; that so far as the surface phenomena are concerned accuracy to any reasonable extent is easily obtainable and there remain only the two subjects mentioned in paragraph 3 above. Their complexity and difficulty must not be underestimated but undoubtedly symmetry and order exist capable of rule and simplification. It is not a subject that can be tackled by anyone with his living to earn. It must be done by a gentleman of leisure or, what is the same thing, the mathematicians and physicists of our Universities and Institutions. There is no money in it! but to those happy beings, independent of such considerations, who live in the cloistered atmosphere of Cambridge or Teddington—or both!—I can honestly recommend the subject as combining the fierce excitement of a jig-saw puzzle with the intellectual isolation of a man playing chess with himself at the North Pole.

MR A. WARMISHAM: I am glad to accept Commander Baker's invitation to describe my practice in designing photographic lenses.

The initial calculations, on a good slide rule or with 4-figure logarithms, adjust

the powers and separations and thicknesses of the several component lenses to get, firstly, the desired focal length, and, in addition, any special feature such as the position of the second nodal point of a telephoto combination.

The material is selected at this stage, regard being paid to the Petzval sum and the chromatic aberrations. The Petzval sum is adjusted to a likely value, dependent upon the type of lens and the extent of angular field required. The chromatic aberrations are dealt with together. The most convenient expression for the oblique colour known to me is the one derived by Dennis Taylor: for the axial colour there are several well-known forms: the sum of the oblique colour terms is adjusted very nearly zero and the sum of the axial colour terms for the range of the spectrum $D-G'$ is adjusted to a likely value, dependent upon the type of lens and the required working aperture. These two chromatic conditions govern the choice of material as to the ν value.

The preliminary work described above is greatly expedited in lenses consisting of separated components by the use of a general theorem established by Dennis Taylor and contained implicitly in his system of "first"* order aberrations. The theorem indicates that for the simultaneous correction of oblique colour, coma, and distortion a generalised symmetry is necessary: in Taylor's notation the quantities α , $\alpha-\beta$, ν must be symmetrical, α being a parameter determining the convergence of the axial pencil at any of the elements and β a second parameter determining the convergence of the chief rays. In the majority of modern anastigmats this generalised symmetry has replaced the older geometrical symmetry about a central point.

In the subsequent work any desired alteration is effected in the main by small changes in shape of the component lenses; such changes have a negligible effect on the aberrations already settled. The "first" order spherical aberration, coma, astigmatism, and field curvature are next calculated for the combination as determined by the preliminary work. Any of the known forms of "first" order aberrations can be used for this: Dennis Taylor's methods are very convenient and rapid, though I know some designers who quarrel with his sign convention, but that is a small matter and easily modified for one's own purposes. The essential feature of a system of "first" order aberrations is that they should lend themselves rapidly to interpretation in terms of circle of confusion in the image plane or some equivalent quantitative criterion.

The combination is modified at this stage to reduce the "first" order aberration terms to *likely* amounts. Finite difference equations connecting change of aberrations with change of shape of each component are powerful aids to this end.

The combination containing known amounts of "first" order aberrations is next submitted to trigonometrical computation, and all the aberrations determined. If the *likely* amounts of "first" order aberrations left in the system were well chosen, they are more or less accurately balanced over the required aperture and field by higher order aberrations. It is generally sound to assume that the usually small outstanding aberrations revealed by the trigonometrical work can be elim-

* This term is here used in the same sense as in Commander Baker's contribution.

inated by altering the "first" order terms an equal amount, the higher order aberrations remaining substantially unchanged. It may, however, be necessary to go further back and make radical alterations in powers and separations if large higher order aberrations are revealed.

I believe most designers of photographic lenses economise in trigonometrical work where possible. For all ordinary work the calculation of coma is confined to three skeleton rays in the meridian plane. If this gives a good result, other things being good, it is time to make a model rather than compute a pencil of skew rays.

MR THEODORE CHAUNDY: I can find little to add to the opener's remarks save by way of emphasising my active agreement with his main contentions.

I have felt very much that the pure mathematician could be taken to task because, while he is indefatigable in exploration of new regions at the bounds of his subject, nearer at home he leaves uncultivated many small plots of supreme interest to this or that sort of physicist. Geometrical optics seems to me one such long-neglected plot. For, while on the one hand we can describe the performance of a given lens-system with most complete assurance and success, yet the reciprocal problem of choosing a lens-system which shall achieve a desired performance can only be attacked in the most primitive and tentative fashion and is, in fact, to the mind of the pure mathematician, still an unsolved problem, once we pass beyond the region of the first approximation or so.

The problem is in one sense peculiar, because no unascertained facts or half-understood principles appear to be involved. The physical laws are simple: they lead to geometry equally elementary. And into the resulting analysis there seems no entry of transcendental functions of advanced character. All is expressible in terms of straightforward surds and polynomials. The difficulty is merely that at each incidence of the ray on a new spherical interface and again at each refraction into a new medium the order of these polynomials is doubled and the algebra becomes more and more unmanageable. It is essentially in the reduction of this huge, sticky mass of algebra to a more digestible form that the pure mathematician is called in, and so far, to my mind, with no shadow of success.

The first and very natural attempt towards simplification consists usually in elimination of the ambiguities (which enter at each incidence and refraction) by expansions in series which are valid as approximations so long as the rays under consideration conform reasonably enough to some standard configuration.

The accepted expansion has always been that of a power-series in the sine or tangent of an angle or perhaps even in the angle itself, this angle ordinarily being the ray's inclination to the optic-axis. Commander Baker and Prof. Filon have now challenged the convenience and the reliability of these expansions in powers. I think that this criticism will mark a very definite advance in our understanding of geometrical optics. After much wrestling with the aberrations, in endeavours to render them more tractable, I have found myself moving towards the same conclusion though perhaps for a different reason. They challenge these power-

expansions on the ground of *validity*. I find them unsatisfactory on the ground of *form*.

I think that progress will only come and geometrical optics grow manageable (if ever) by discovery of some type of expansion more intrinsically suited to it. As a pure mathematician I like to believe that some such type of series, as elegant, let us say, as a Fourier series, is waiting to be discovered for this purpose. Perhaps it may involve elliptic functions—for they seem a little indicated by the sine-law of refraction. Perhaps—and much more probably—we ought to look for an expansion not into a mere sum of terms but into an expression in which addition and multiplication, for instance, are equally involved. Commander Baker and Prof. Filon in their recent paper read to the Royal Society suggest the likelihood of an expansion into a continued product or a continued fraction. I think that remark may be very pertinent and I trust that their further work on the subject may succeed in disentangling for us the vital formulae.

DR J. W. FRENCH: If Commander Baker can persuade the mathematicians of this country to interest themselves, not spasmodically but seriously and continuously, in the art of optical computation, whether algebraic or trigonometrical, the whole industry will be greatly indebted to him, and grateful to the mathematicians.

Industry is frequently scolded for its neglect of Science. It is a theme around which excellent addresses that are most likely to be fully reported in the Press can be easily composed. To what extent is the complaint justified? Does any well-informed person really believe that painstaking and effective research is non-existent in the numerous highly organised factories of Britain? The optical industry is comparatively small. It comprises a few large firms and a larger number of very small ones. The large firms at least are well organised. If the amount expended by these firms in experimental and research work was published, many persons would be surprised.

In the past the optical industry has not neglected Science. On the contrary, it is the University that has neglected the optical industry. For this neglect there is no doubt sufficient excuse when the circumstances are considered. The optical industry is small and offers correspondingly little scope. It is not in the fortunate position of the electrical industry. Physical optics as a subject of University study has more fascination than optical computation. It is not easy to relieve the monotony of workshop optical computation. The last highly qualified mathematician whom I engaged to do what Commander Baker is advocating returned to academic work after a month or two. To train a man and lose him before he starts production is a serious item of expense. It is well that the mathematicians should understand that they are being asked to engage in a most difficult and laborious undertaking with a prospect of remuneration that is indefinite and probably small.

Commander Baker has stated the case for an algebraic system in the expectation, doubtless, that the workshop methods would be discussed by the manufacturers. For many reasons I think it unlikely that any algebraic system will wholly

supplant the trigonometrical method of tracing individual rays through the system. On the other hand, the one system may be supplemented by the other with great advantage, and it is in devising algebraic formulae to facilitate the requisite adjustments of the system indicated by the ray tracing processes that progress is to be expected.

Trigonometrical ray tracing is, I understand, objected to on the ground that it is a trial and error method. However true that may be, it is certainly not blind trial and error, for the tracing of individual rays gives the computer a very clear and simple idea of every part of the system. It is claimed for algebraic systems that they are more direct, especially in the computation of a combination for which there is no precedent. Some trial and error, however, is inevitable, whatever the system employed. Most frequently in practice the design of an optical combination is based upon that of some existing combination. It is rarely the case that the designer has no precedent to help him far on his way. Under these circumstances I believe that, in the hands of a workshop computing staff, the desired results will be obtained more accurately and quickly by ray tracing methods when supplemented by algebraic methods and not displaced by them.

In the conclusion of his introductory remarks Commander Baker appeals for mathematical precision tools to replace the "file and the hammer and the hacksaw that form our present equipment." Recently I saw apprentices limited to the use of these simple tools produce sliding parts of microscope mounts as accurate as any ever produced by means of precision tools, though not so rapidly. When properly applied, the file and hammer and hacksaw may be most excellent tools and their optical equivalents are not to be despised.

Surely it is not suggested that the present equipment of the optical computer is inadequate so far as accuracy is concerned. Ray tracing is almost universally adopted in practice because of the precision that is attainable. The precision in ray tracing is limited by the accuracy of the conversion tables and by that of the logarithmic tables when machines are not employed. The precision of the algebraic method is limited by the size of the formulae which express the aberrations. At the present time the algebraic formulae of even a moderate order are very unwieldy.

I understand the position to be that the mathematician is being asked to devise mathematical precision tools that will more nearly approximate to the accuracy of the existing file and hammer trigonometrical ray tracing appliances at present used by us and that will be more rapid. It is quite true that ray tracing is a laborious process. An individual must find the work very monotonous; but in the factory the work can be subdivided. Girls perform quite happily the routine operations, which in ray tracing are extremely simple, their efforts being suitably directed by the head computer to whom the results are submitted.

Complicated algebraic formulae may give excellent results when applied by a mathematician and particularly by the mathematician who has evolved them, and yet be unusable in the hands of less highly trained people.

Much labour has already been expended in the evolution of algebraic formulae.

The principal investigations are outlined in *The Formation of Optical Images* compiled by von Rohr and translated by Mr Kanthack. More complete and sometimes more practical information is obtainable from the back numbers of the *Zeitschriften für Instrumentenkunde*. The volumes for 1898, 1899, and 1900 contain, for example, the approximate formulae of Harting. The mathematician should, therefore, be able to read German. The foundation upon which a good deal of the more recent work is based can be studied in Euler's *Dioptricae* of which there are three volumes written in Latin between the years 1769 to 1771. Euler's work was condensed and put into a more practicable form by Klügel, whose *Analytische Dioptrik*, published at Leipzig in 1778, is well worth reading. The numerical examples are particularly instructive and in this connection the following translation of the concluding paragraph of Klügel's preface is of interest: "Now I shall add not another word to this long preface except to assure the reader that the book has been published under my personal supervision and to hope that the reader will be satisfied with the accuracy and with the clearness of the print. It is obviously not impossible that one or two errors, particularly in the numerical calculations, may have been overlooked...." Freedom from such errors is not a characteristic of many more modern works.

Coddington's famous system of optics which was published in 1829 was based upon the work of Young and Airy. According to von Rohr's *Formation of Images*, "Coddington's merit lies in his lucid presentation of the subject, which is greatly enhanced by a very clear notation and excellent printing." However true that may be, Coddington's work, unlike Klügel's, is spoiled, so far as the practical optician is concerned, by numerous errors. To use many of the formulae as they stand would be disastrous in practice. The unfortunate manufacturer would require to edit the work before applying it, and this is hardly the function of the manufacturer! I sincerely hope that any mathematician who is so kind as to come to the assistance of the industry will not neglect this aspect of the question. Why is Coddington's book defective in this practical respect? Is it not an indication of the regrettable disregard of practice by science?

It is characteristic of Newton that he combined theory and practice. Most of the old masters did so, realising that personal experience is the only reagent that can resolve the doubtful elements of theory. The opposite school, of which Coddington's work is suggestive, concerns itself with the mathematical aspect of optics to the exclusion of its applications, in which they are not interested. Mathematical elegance takes precedence of precision and those problems of optics that do not lend themselves to elegant mathematical treatment are generally ignored.

To satisfy the needs of the industry it is essential that all the details of the problem should be dealt with, regardless of their mathematical attractiveness. It is not sufficient to deal only with simple doublet objectives of moderate angular aperture, the calculation of which involves no real difficulty to the computer. It is essential, further, that the results should be tested by actual construction. If the result is unsatisfactory, as may quite likely be the case in pioneer work, the causes must be investigated and the design modified accordingly.

The pioneer designer must not be unduly disposed to blame the workshop for inaccurate production or for the use of wrong materials. The more inexperienced a designer, the more reluctant is he likely to be to scrap a pretty piece of calculation or drawing, or to recognise that the faults may lie in his work and not in that of the shop. Suppose some mathematician took up the mathematical problems of the optical industry. Could he do so successfully by remaining in his study? Would it be possible to communicate clearly to him the problems and conditions? I do not think so. Peter Barlow in 1827 published his *Rules and principles for computing the radii of curvature for achromatic object glasses* and to the title he added the words "submitted to the test of experiment." Untried formulae have but little practical value. Is it likely that a chef who had never entered a kitchen and applied the test of tasting could produce an appetising dish?

The first move of the mathematician must, therefore, be from the study right amongst us, where he can take the temperature and feel the pulse of the industry, survey the conditions, diagnose such maladies as may exist, and elaborate his cures at his leisure. Any mathematician who cared to spend several months of his annual vacation in the factory would probably find the work much more fascinating than he anticipated.

From such a combination of the workshop and the University, which Commander Baker evidently desires, much good must surely result.

There are difficulties, of course. Some manufacturers would not willingly admit to their confidence anyone who might use the information so gained solely in the service of some competitor. Trade discretion of this kind is inevitable. This objection could hardly apply to a mathematician who is permanently engaged in teaching, whose academic position is already assured, and who, in entering the factory, has no intention of abandoning academic life. There is some danger also that the mathematician may find factory methods more highly developed and effective than is generally supposed and be disposed to accept the trigonometrical system of computation rather than attempt the laborious work of elaborating new algebraic methods. If both the manufacturers and the mathematicians can maintain an atmosphere of vigorous enthusiasm, all these minor difficulties will certainly be dissolved.

ACHROMATISM

By H. W. LEE, B.A.

(Messrs Taylor, Taylor and Hobson, Ltd.)

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ABSTRACT. Various expressions for achromatism are correlated and the usual conditions for achromatism are shown to depend on the tracing of two rays through the system.

PRELIMINARY

IN the following, consideration is limited to systems surrounded by air. Such a system is defined by the equivalent focal length (or by the power), and the positions of the two nodal points. All these data are subject to chromatic aberration.

Let δp , δq , δf , δv be the chromatic aberrations of the two nodal points, the focal length and the focussing distance respectively. The object is supposed free from aberration. From the fundamental equation

$$\frac{1}{v} - \frac{1}{u} = F,$$

we have

$$\frac{\delta v - \delta q}{v^2} + \frac{\delta p}{u^2} = -\delta F \quad \dots\dots(i).$$

If $u = \infty$, $v = f$ and $\delta v_{\infty} - \delta q = +\delta f$.

So that if $\delta v_{\infty} = 0 = \delta f$ then $\delta q = 0$.

But if $\delta v = 0$ for all values of v we must have $\delta p = 0$.

From (i) it is seen that if $\delta v = 0$ for any two positions of the object this further condition is satisfied.

Since the magnification

$$m = 1 - vF, \\ \delta m = -v\delta F - F(\delta v - \delta q) \quad \dots\dots(ii),$$

so that there will be no chromatic difference of magnification.

The three conditions for complete achromatism are therefore

- (1) Achromatism of the focal length (or of the power).
- (2) „ „ focussing distance for *any* two rays.

In a symmetrical system $\delta p = -\delta q$ so that if condition (1) is satisfied ($\delta f = 0$), together with condition (2) for *one* ray initially parallel to the axis ($\delta v_{\infty} = 0$), complete achromatism is secured. But it is sufficient for these conditions to be satisfied by the front half alone, for

$$F = 2F_1 - dF_1^2,$$

where F_1 relates to either half and d is the separation of the adjacent nodal planes so that

$$\begin{aligned}\delta F &= 2\delta F_1 - 2dF_1\delta F_1 - F_1^2\delta d \\ &= 2\delta F_1(1 - dF_1) + 2F_1^2\delta d \\ &= 0 \text{ if } \delta F_1 = \delta v_{1\infty} = 0 = \delta q_1.\end{aligned}$$

Also
$$l_3^2\left(\frac{1}{v}\right) = l_1^2\delta F_1 + l_3^2\delta F_1 + l_1^2F_1^2\delta d \text{ (see equation (iii } a) \text{ below)}$$

$$= 0 \text{ again.}$$

In many systems for which algebraic approximations are useful, e.g. telescopic systems and photographic lenses, it is sufficient if achromatism is obtained for a distant object, we then merely achromatise v_∞ and f and there will then be no colour error in magnification. We see that in general a symmetrical system will exhibit a higher degree of colour correction than an unsymmetrical one.

In obtaining expressions for the achromatic conditions in terms of the constituents of a system, we shall use the notation of Chalmers* and the following may be considered a corollary to his paper on "Monochromatic Aberrations†."

1. ACHROMATISATION OF THE FOCUSING DISTANCE.

We have
$$l_1F_{1...2r+1} = \sum_1^{2r-1} lF + (lF)_{2r+1},$$

$$\therefore l_1\delta F_{1...2r+1} = (l\delta F + F\delta l)_{2r+1} \text{ due to } (2r+1) \text{ alone}$$

and

$$v_{2r+1} = (l/l_1F)_{1...2r+1},$$

$$\therefore \delta v_{2r+1} = -\frac{1}{l_1F} \delta F_{1...2r+1} + \frac{\delta l}{l_1F},$$

where the upright characters refer to another colour.

Or
$$l_1^2(FF)_{1...2r+1} \delta v_{2r+1} = -l\delta F_{2r+1} + l_1F_{1...2r-1} \delta l$$

$$= -l\delta F_{2r+1} - l_1^2(FF)_{1...2r-1} \delta d_{2r}.$$

Expressed at the end of the system δv must be multiplied by

$$m_{2r+1...2n+1}^2 = (F_{1...2r-1}/F_{1...2n+1})^2,$$

and since $l_1F = l/v$ we obtain finally

$$l\delta(1/v) = \Sigma l\delta F + l_1^2 \Sigma (FF)_{1...2r-1} \delta d_{2r} \dots\dots(iii).$$

If variation of thickness is neglected this expression is the same as that given by Southall‡ and by Koenig§.

In a system where the separations are small so that l is not substantially different from l , the equation becomes

$$l^2\left(\frac{1}{v}\right) = \Sigma l^2\delta F + l_1^2 \Sigma F^2_{1...2r-1} \delta d_{2r} \dots\dots(iii a),$$

with sufficient exactitude. This is identical with Dennis Taylor's|| when it is remembered that

$$l_{2r+1}/l_{2r-1} = v_{2r+1}/u_{2r+1},$$

so

$$l_{2r+1}/l_1 = v_{2r+1} \cdot v_{2r-1} \dots / u_{2r+1} \cdot u_{2r-1} \dots$$

* *Trans. Opt. Soc.* 18 (1917), 3.

† *Proc. Phys. Soc.* 30 (1918), 100-118.

‡ *Geometrical Optics*, p. 518.

§ *Theorie d. Opt. Inst.* (ed. v. Rohr), p. 347; Translation (Kanthack), p. 353.

|| *Applied Optics*, pp. 285-286.

The complete expression in v. Rohr* can also be readily reduced to (iii). Translated into Chalmers' notation, Koenig's equation is

$$n\delta\left(\frac{1}{v}\right) = \Sigma n Q_{2r+1} \left\{ \frac{\delta n}{n} \right\}_{2r+2} - \frac{\delta n}{n} \right\}_{2r},$$

where

$$Q = \frac{n_{2r+2}}{r} - \frac{1}{v_{2r+1}} = \frac{n_{2r}}{r} - \frac{1}{u_{2r+1}},$$

being invariant on refraction.

$$\text{I.e.; } n\delta\left(\frac{1}{v}\right) = \Sigma n \left(\frac{\delta n_{2r+2}}{r} - \frac{\delta n_{2r}}{r} - \frac{\delta n_{2r+2}}{v_{2r+1} n_{2r+2}} + \frac{\delta n_{2r}}{u_{2r+1} n_{2r}} \right) \dots\dots(a).$$

Now

$$\frac{\delta n_{2r+2}}{r} - \frac{\delta n_{2r}}{r} = F - F = \delta F.$$

In the remainder of the right-hand side the terms containing δn_{2r} are

$$\begin{aligned} & - \frac{\delta n}{n} \left(\frac{n_{2r-1}}{v_{2r-1}} - \frac{n_{2r+1}}{u_{2r+1}} \right) \\ & = - \frac{\delta n}{n} \left(v_{2r-1} - u_{2r+1} \right) l_1^2 FF \text{ as } l_1 = l_1 \\ & = - d. \delta n/n. l_1^2 FF = l_1^2 FF \delta d. \end{aligned}$$

So equation (a) reduces to (iii).

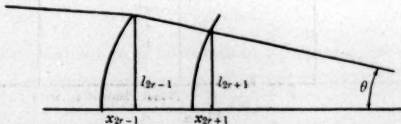


Fig. 1

A. E. Conrady† has expressed the achromatic condition for any ray in a remarkably elegant and convenient form for using with trigonometrical computation. His condition is

$$\Sigma (t - t') \delta n = 0,$$

where t' is the length of the path of the ray in the medium n and t is the corresponding axial length. Consider the path of the ray between the surfaces $(2r - 1)$ and $(2r + 1)$. The contribution of this part to the sum is $(t - t') \delta n$.

Now

$$t' \cos \theta = t + x_{2r+1} - x_{2r-1},$$

where θ = inclination of the ray to the axis, and x the sagitta of the surface (Fig. 1),

$$\therefore t' - t = x_{2r+1} - x_{2r-1} + \frac{1}{2} t. \theta^2$$

since t can be written for t' in the small term.

Further

$$x = l^2/2r \text{ and } \theta = l_1 F_{1\dots 2r-1}/n_{2r},$$

so $(t' - t) \delta n = -\frac{1}{2} (l^2 \delta F_{2r+1} + l^2 \delta F_{2r-1}) + l_1^2 F_{1\dots 2r-1}^2 \delta d_{2r}$ where $d = t/n$,

$$\therefore \Sigma (t - t') \delta n = \frac{1}{2} (\Sigma l^2 \delta F + l_1^2 F^2 \delta d)$$

$$= \frac{1}{2} l^2 \delta \left(\frac{1}{v} \right).$$

* Loc. cit. p. 341.

† Monthly Notices, R.A.S. (Jan. 1904).

This shows what meaning is to be attached to any departure of the sum $\Sigma (t - t') \delta n$ from zero, and our condition is identical with Conrady's in the paraxial region to which we have tacitly confined ourselves. In the above expressions δF is of course F/ν and

$$\delta d = -\frac{t}{n} \delta n = -\frac{d}{n} \delta n.$$

2. ACHROMATISM OF THE FOCAL LENGTH.

It is not possible to give an expression for δF in terms of the constants of the system for one ray, but as complete achromatism requires the tracing of two rays this does not greatly matter. Suppose that the second ray traced traverses the system in the reverse direction from infinity, and that the l so obtained is written l' so that our complete requirements are

$$l^2 \delta V = l'^2 \delta V' = \delta F = 0.$$

We can express δF in terms of the l, l' and F 's. The following demonstration is due to Chalmers.

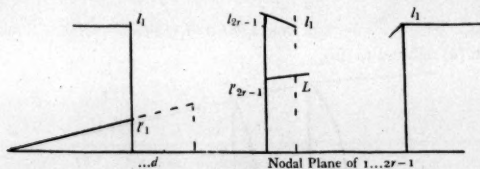


Fig. 2

Consider the system divided into two parts ($1 \dots 2r+1$) and ($2r+1 \dots 2n+1$) and let the nodal plane of the system ($1 \dots 2r+1$) be intersected by the reverse ray at height L (Fig. 2), then

$$l_1 F = l_1 F_{2r+3\dots} + L F_{1\dots 2r+1}.$$

Now

$$\frac{l'_{2r+1} - L}{F_{2r+3\dots}} = \frac{l_1 - l_{2r+1}}{F_{1\dots 2r+1}},$$

so

$$l_1 F = l_{2r+1} F_{2r+3\dots} + l' F_{1\dots 2r+1} \quad \dots (b).$$

Due to variation of F_{2r+1} only, we have

$$l_1 \delta F = \frac{l_1 \partial F}{\partial F_{2r+1}} \cdot \delta F_{2r+1} + F_{2r+3\dots} \delta l_{2r+1},$$

$$\begin{aligned} \therefore l_1^2 \delta F &= l_1 F_{2r+3\dots} \delta l + l' \left(l + F_{2r+1} \frac{\partial l}{\partial F_{2r+1}} \right) \delta F_{2r+1} \\ &= (l' \delta F)_{2r+1} - l_1^2 F_{2r+1\dots} F_{1\dots 2r-1} \delta d_{2r}, \end{aligned}$$

so adding up for variations of all the surfaces

$$l_1^2 \delta F = \Sigma l' \delta F - l_1^2 \Sigma F_{1\dots 2r-1} \cdot F_{2r+1\dots} \delta d \quad \dots (iv),$$

a very similar equation to (iii).

F' may be expressed in terms of l' and F from (b) and l' in terms of l from the following identity*

$$\frac{y_{2r+1}}{l_{2r+1}} = \frac{y_1}{l_1} - \frac{l_1 y_1}{u_0} \Sigma \frac{d}{l},$$

where y corresponds to l for any ray not initially parallel to the axis.

In the particular case of the second ray proceeding from the front focal point, this becomes

$$\frac{l'}{l} = \frac{l'_1}{l_1} + d_0 F,$$

and l' can be substituted in (iv).

If we neglect variations of thickness with n , we have

$$l_1^2 \delta F = \Sigma l l' \delta F \quad \dots\dots (iv \ a)$$

$$= \frac{l'_1}{l_1} \Sigma l^2 \delta F + F \Sigma l^2 (d_0 \delta F)$$

$$= \frac{l'_1}{l_1} l^2 \delta \left(\frac{1}{v} \right) + F \Sigma l^2 d_0 \delta F \quad \dots\dots (v),$$

i.e. $\delta F = 0$ is equivalent to the new condition

$$\Sigma l^2 d_0 \delta F = 0 \quad \dots\dots (vi),$$

when the condition $\delta \left(\frac{1}{v} \right) = 0$ has been satisfied.

The condition (iv a) can easily be seen to be a particular case of a more general condition.

$$\text{For} \quad y l = \frac{y_1}{l_1} l^2 - \frac{y_1}{l_1 u_0} l^2 d_0.$$

$$\begin{aligned} \text{So} \quad \Sigma y l \delta F &= \frac{y_1}{l_1} (\Sigma l^2 \delta F) - \frac{y_1}{l_1 u_0} \Sigma l^2 d_0 \delta F \\ &= 0 \text{ if } \delta \left(\frac{1}{v} \right) = \delta F = 0, \text{ by (vi),} \end{aligned}$$

and this condition can replace either of the others. The second may be any ray whatever and not restricted to that from the front focal point (Chalmers) or to that passing through the centre† of the stop (Kerber). The condition $\Sigma y l \delta F = 0$ can thus be considered a generalisation of previously enunciated particular cases.

This theorem can be derived differently. Consider a ray cutting the axis at any point x . The image of this point is formed in the object space at a distance

$${}_1 d_x = l_1^2 \Sigma_1 \frac{d}{l}$$

from the front surface, the l 's being reckoned for parallel incident light.

Also the image of the surface $(2r+1)$ is at a distance ${}_1 d_{2r+1}$. Hence if θ = inclination of the ray through x to the axis, the intercept of the image ray on the

* Chalmers, *Trans. Opt. Soc.* 18 (1917), 136, equation xviii b; v. Rohr, *Theorie d. Opt. Inst.* p. 146 (3); Translation, p. 146 (3).

† A. Kerber, *Z. f. Instrk.* 35 (1915), 24.

image surface is $x d_{2r+1} \tan \theta$. Now the magnification of the surface by the preceding system is l_{2r+1}/l_1 , so the real intercept on this surface is

$$(y) = \frac{l_{2r+1} \tan \theta}{l_1} x d_{2r+1}.$$

So

$$\begin{aligned} \Sigma y \delta F &= \frac{\tan \theta}{l_1} \Sigma l^2 d \delta F \\ &= 0 \text{ by (vi)} \end{aligned} \quad \dots (vii).$$

That this condition implies correction of the size of the image immediately follows. Consider the principal ray on surface $(2r+1)$, crossing the axis at a distance v from the surface at a height

$$y = l/l_1 \cdot d \tan \theta = \frac{v \cdot i}{v - v'},$$

where i = the size of the image after refraction at $(2r+1)$ where achromatism is assumed for the paraxial region (Fig. 3).

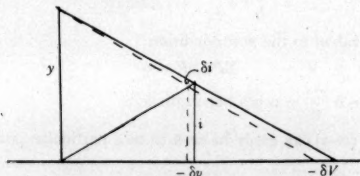


Fig. 3

Now

$$\frac{\delta i}{\delta v} = \frac{i}{v - v'} = \frac{y}{v},$$

and

$$\frac{\delta v}{\delta v} = \frac{v}{v},$$

and since

$$\frac{1}{v_{2r+1}} - \frac{1}{v_{2r+1} - d} = F_{2r+1},$$

$$\frac{\delta v}{v^2} = -\delta F \text{ neglecting } \delta d,$$

so

$$\delta v = -v v \delta F,$$

$$\therefore \delta i = -v y \delta F = \frac{\tan \theta}{l_1} \cdot l v d \delta F.$$

At the end of the system this becomes $\frac{\tan \theta}{l_1^2 F} \cdot l^2 d \delta F$.

Hence, adding up for the system $\delta i = 0$ if $\Sigma l^2 d \delta F = 0$.

H. D. Taylor* gives a different expression for the magnification error which can be derived thus.

* *Applied Optics*, p. 297, xii, and p. 300, xiv.

We have

$$\delta i_{2r+1} = \frac{idv}{v-v}$$

due to surface $(2r+1)$.

Adding the previous aberration

$$\delta i_{2r+1} = \frac{idv}{v-v}_{2r+1} + \delta i_{2r-1} \cdot m_{2r+1}.$$

Now

$$\begin{aligned} u_{2r+1} &= v_{2r-1} - d_{2r}, \\ \therefore \delta u_{2r+1} &= \delta v_{2r-1} - \delta d_{2r} \\ &= -(v\delta F)_{2r-1} - \delta d_{2r}, \\ \therefore \left(\frac{\delta i}{i}\right)_{2r-1} &= \frac{\delta u}{u-u}_{2r+1} = \frac{\delta u_{2r+1}}{(v-v)_{2r-1}} \\ &= -\frac{v \cdot v}{v-v} \delta F + \frac{\delta d}{v-v}. \end{aligned}$$

So

$$\begin{aligned} \frac{\delta i_{2r+1}}{i_{2r+1}} &= \frac{\delta v}{v-v}_{2r+1} - \left(\frac{v\delta F}{v-v} \delta F - \frac{\delta d}{v-v} \right)_{2r-1} \\ &= -\frac{v \cdot v}{v-v} \delta F + \frac{\delta d_{2r}}{(v-v)_{2r-1}}. \end{aligned}$$

In the final image

$$\begin{aligned} \frac{m + \delta m}{m} &= \Pi \left(\frac{m + \delta m}{m} \right)_{2r+1} = \Pi \left(\frac{i + \delta i}{i} \right)_{2r+1} = 1 + \Sigma \left(\frac{\delta i}{i} \right)_{2r+1}, \\ \therefore \frac{\delta m}{m} &= -\Sigma \frac{\delta F}{\frac{1}{v} - \frac{1}{v}} + \Sigma \frac{\delta d_{2r}}{(v-v)_{2r-1}} \quad \dots\dots(viii). \end{aligned}$$

The first term on the right-hand side is Taylor's expression $\Sigma \frac{2}{(\alpha - \beta)v}$ and the second his parallel plate correction.

Now $\frac{1}{v} - \frac{1}{v} = \frac{l_1 y_1}{l \cdot y} R$, where $\frac{1}{u_1} - \frac{1}{u_1} = R_1$,

so $v - v = \frac{v \cdot v}{l y} l_1 y_1 R_1$
 $= \frac{l_1 y_1 R_1}{l_1^2 F F},$

and (viii) becomes $\frac{\delta m}{m} = -\frac{1}{l_1 y_1 R_1} (\Sigma l y \delta F - l_1^2 \Sigma F F \delta d).$

In the case of the two rays coming from infinity in opposite directions, y becomes l' and

$$l_1 y_1 R_1 = l_1^2 F,$$

and

$$\begin{aligned} \left(\frac{\delta m}{m} \right)_{m=0} &= \frac{\delta F}{F} \\ &= 0 \text{ if } \delta F = 0. \end{aligned}$$

DISCUSSION

Mr P. F. Everitt said that the author had rendered a useful service in reducing to a common notation the expressions given by various authorities and bringing them together in a single paper. As a practical computer he himself preferred to use methods of a different character in correcting systems for colour. He thought it should be realised that actual systems of appreciable aperture were not, and should not be, corrected for paraxial rays. The larger the aperture the greater the departure from paraxial correction would be. It was a common experience that in a well-corrected symmetrical photographic double objective the separate halves were not well corrected, and if the halves were corrected separately the whole lens would prove unusable.

Mr T. Smith (partly communicated) disagreed with the author's statement that a symmetrical double lens would be corrected if the first half were corrected for an object at infinity. Admittedly only two conditions were required, but if these did not involve the separation of the two components the correction would be maintained however far apart they might be placed. Taking an object at infinity the image produced by the first lens is according to the author free from aberration and so is the image of this produced by the second lens, whatever its position, since the complete lens is corrected. That is to say the satisfaction of two conditions would give complete correction for a single component, contradicting the previous conclusions that three conditions were necessary, since the component need not itself be symmetrical. The correct conditions were that the image of the axial point midway between the components should be free from aberration, and that there should be no chromatic variation in the position of the first principal focus of the single component, corresponding to an incident parallel beam on the inner surface of the lens. Similar conditions apply to a hemi-symmetrical lens. They may be proved as follows. Let the two components be similar to a lens of focal length f , with focal distances u and v , the former on what is the external side of the double lens, the latter on the internal side. Let the distance of the stop centre, which in the complete system will be the centre of geometrical symmetry, be distant t from the lens on the internal side. Then if two similar lenses are combined in the way suggested, the first having a focal length f/n and the latter a focal length nf , the corresponding focal and stop distances are obtained by introducing the factors $1/n$ and n . Let F be the focal length of the combination, and V the distance from the last surface to its principal focus. Both V and F must maintain the same values for different colours. Now the separation of the internal principal foci of the two components is

$$\left(n + \frac{1}{n}\right)(v - t),$$

and therefore by a very well-known formula

$$F\left(n + \frac{1}{n}\right)(v - t) = f^2.$$

Thus $f^2/(v-t)$ must not change with colour, and this is the focal distance of the image of the stop centre in the typical component. The corresponding differential condition $2\delta f/f = \delta v/(v-t)$ is a simple equation for t . To prove the other condition consider a beam of light parallel to the axis between the components. It will come to a focus at a distance nu from the last surface. This beam corresponds to the magnification $-n^2$ for the complete lens, and therefore

$$nu = V + n^2 F$$

giving the second condition that u must be constant as the colour changes. On taking the two results together it is seen that the position of the stop image must be invariable.

The various expressions given by the author are applications to paraxial rays of a simple general result which applies to any rays and may conveniently form the basis of investigations of aberrations of all kinds in a centred system of surfaces. Denoting the refractive index by μ , the direction cosines of a ray by L, M, N , the power of the surface for the ray by K (which is equal to the curvature multiplied by $\mu\mu'$ times the sine of the deviation divided by the optical invariant $\mu \sin \phi$), the point of refraction referred to the vertex of the surface by x, y, z , and the length of the path between two surfaces by t , the y and z equations of refraction are of the form

$$y_p K_p + \mu_p M_p - \mu_{p-1} M_{p-1} = 0$$

and the geometrical relations are

$$y_{p+1} - y_p - t_p M_p = 0.$$

Denote corresponding quantities for another ray by accented letters. Then

$$\begin{aligned} 0 &= \Sigma y_p (y'_p K'_p + \mu'_p M'_p - \mu'_{p-1} M'_{p-1}) - \Sigma \mu_p M_p (y'_{p+1} - y'_p - t'_p M'_p) \\ &\quad - \Sigma y'_p (y_p K_p + \mu_p M_p - \mu_{p-1} M_{p-1}) + \Sigma \mu'_p M'_p (y_{p+1} - y_p - t_p M_p) \\ &= \Sigma y_p y'_p (K'_p - K_p) + \Sigma \mu_p \mu'_p M_p M'_p \left(\frac{t'_p}{\mu'_p} - \frac{t_p}{\mu_p} \right) \\ &\quad - \mu'_0 M'_0 y_0 + \mu_0 M_0 y'_0 + \mu'_n M'_n y_{n+1} - \mu_n M_n y'_{n+1}, \end{aligned}$$

where the first summation is taken over all the surfaces, the second over all elements of the path between the chosen end points, and the y 's in the last line are arbitrarily selected initial and final points on the rays. If all the end points are in the plane $y = 0$ all the terms in this line vanish. Comparison with (iii) shows that that expression holds only for an object at infinity, as otherwise the inclination of the ray is not measured by the power alone. Condition (iii) results from putting $M_0 = M'_0 = y_{n+1} = y'_{n+1} = 0$, and (iv) from $M_0 = M'_n = 0, y_0 = y'_0$. Many interesting variations are easily found, as by the substitutions

$$\mu_p M_p K_{1,n} = \mu_0 M_0 K_{p+1,n} + \mu_n M_n K_{1,p}, \quad y_p K_{1,n} = \mu_0 M_0 \frac{\partial K_{p,n}}{\partial K_p} - \mu_n M_n \frac{\partial K_{1,p}}{\partial K_p}$$

or

$$\mu_p M_p \frac{\partial^2 K_{1,n}}{\partial K_1 \partial K_n} = y_1 \frac{\partial K_{p+1,n}}{\partial K_n} - y_n \frac{\partial K_{1,p}}{\partial K_1}, \quad y_p \frac{\partial^2 K_{1,n}}{\partial K_1 \partial K_n} = y_1 \frac{\partial^2 K_{p,n}}{\partial K_p \partial K_n} + y_n \frac{\partial^2 K_{1,p}}{\partial K_1 \partial K_p}.$$

Professor Cheshire drew attention to the importance of the publication of the work done by industrial workers in the interests of optical science in this country, and he very much hoped that the example set by the author of the present paper would be followed by many others.

The author communicated the following reply: With regard to the symmetrical lens my conclusion is that the complete lens is more strictly achromatic than the half member, and not that the half is completely corrected if the whole is. It is well known, as Mr Everitt said, that this is so in practice.

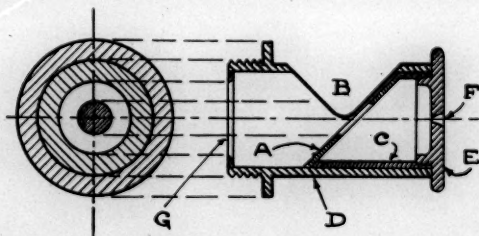


NOTE ON AN EYEPiece FOR TESTING THE SQUARING-ON OF TELESCOPE OBJECT GLASSES

BY PROFESSOR F. J. CHESHIRE, C.B.E., F. INST.P.

Read 9th June, 1921.

THIS eyepiece, as shown by the drawing, screws into the standard sized draw-tube of the English telescope in place of an ordinary eyepiece. The eyepiece itself consists of a plane reflector *A*, preferably of silver, or German silver, mounted at an angle of 45° to the axis of the eyepiece, which, when in position, is illuminated from the sky, or a piece of white paper, through an opening *B* in the side of the tube. This reflector *A* is sweated to the end of a short tube *C*, adapted to slide in



the outer tube *D*. The end of this tube *C* is closed with a disc *E* in the centre of which is a pinhole *F*. The hole in the centre of the mirror must be bored out in the lathe so that it projects accurately as a concentric circle in a plane normal to the axis of the instrument. The inner end of the tube *D* is fitted with a diaphragm *G*.

In use the eyepiece described is fixed in the telescope to be tested, the object glass of which is capped or covered with a cloth. The mirror is then illuminated from the side so that upon looking down the telescope, through the pinhole, a number of images of the illuminated annulus of the mirror will be seen, tailing away generally to one side or the other. This tailing-away indicates that the axis of the object-glass does not pass through the pinhole. The object-glass therefore must be tilted until the various images seen through the pinhole are concentric. When this has been effected the object-glass is accurately squared on and its axis passes through the centre of the eyepiece. Should a component of the objective happen

to be decentered with respect to the other components it will be found to be impossible to get all the images concentric with one another.

When used without a telescope tube—as on an optical bench for example—the eyepiece itself must first be squared on to the object-glass. This can be done with sufficient accuracy by adjusting the eyepiece until the object-glass mount, when seen through the pinhole, is concentric with the diaphragm *G*. When this adjustment has been made the examination should proceed as described above.

The provision of the pinhole is important because by its means all the images, no matter at what distance they may be from the observer, are seen practically equally in focus.

The eyepiece enables squaring-on to be effected with at least equal accuracy to that obtained by the observation of a star image.

A sample eyepiece was exhibited.

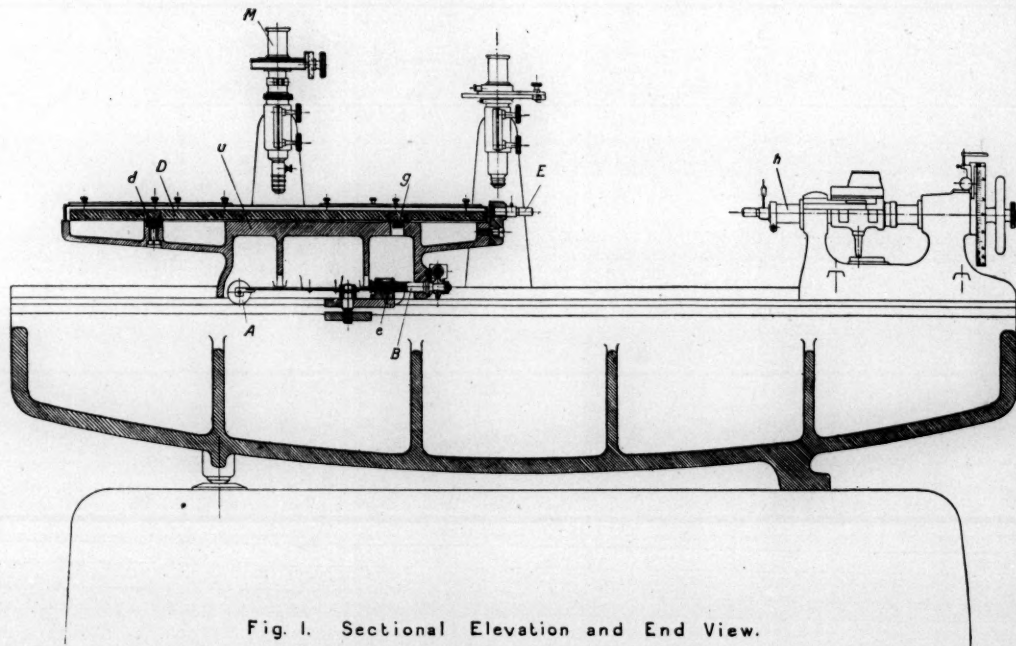
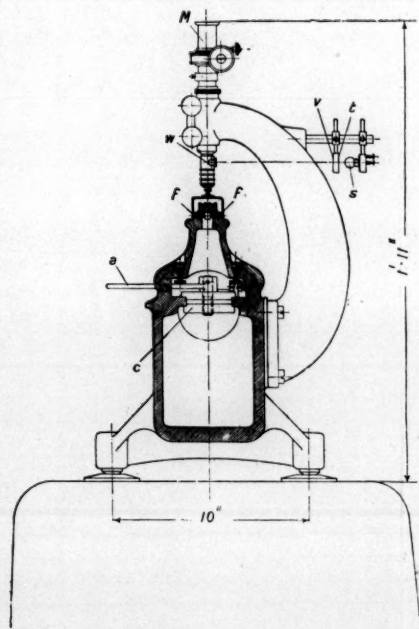


Fig. 1. Sectional Elevation and End View.

THE S.I.P. UNIVERSAL MEASURING MACHINE

By W. L. CUSTANCE

(Communicated by Professor A. F. C. POLLARD.)

Read and discussed, 9th June, 1921.

THE Société Genevoise d'Instruments de Physique, since its foundation in 1861, has almost entirely devoted its attention to the production of instruments of high precision. It has a world-wide reputation for the production of line standards, having made them for every Bureau of Standards of importance throughout the world. The production of these line standards has been the basis of more than one kind of measuring instrument of high precision. The production of large quantities of interchangeable parts brought into use various forms of gauges which have to be accurately checked. For this purpose an instrument giving a finer degree of accuracy than $1/10,000$ inch is needed. There are various forms of machines on the market for this purpose, but most of them merely compare, inasmuch as they are used for comparing a standard piece with the piece to be measured. It is therefore necessary to know the exact length of the standard piece at the time of using it and not only what it measured when it was first put into use, as such standard pieces must necessarily suffer some alteration due to being constantly used. A genuine measuring machine is one by means of which it is possible to determine the absolute length of a piece of work or gauge, with reference to a standard scale which is an integral part of the machine.

The Société Genevoise commenced to devote their attention to the production of measuring machines suitable for engineering laboratories and workshops about 20 years ago, and the machine in its present form is the result of gradual improvements on the early types. The machine is designed in three sizes for the purpose of measuring lengths up to 20", 40" and 80" respectively. (The one we have on demonstration is for measuring lengths up to 20".) The machine consists of four principal parts:

- (1) the cast iron bed made of such a section as to give minimum flexure under its own weight,
- (2) a slide carriage which supports the graduated scale and can be made to move along the bed,
- (3) a micrometer headstock fixed at the right-hand end of the bed which serves as a contact pressure indicator and as a means of setting the zero before commencing a set of observations, and
- (4) the micrometric microscope which is carried in a bracket attached to the rear portion of the bed and enables the reading of the displacement of the carriage on the standard scale.

The carriage *U* (Fig. 1) is of cast iron and rests on the surface of the bed which is in the form of a V and flat. The bulk of its weight, however, is taken on

four rollers *A* which are mounted on springs underneath the carriage and serve to reduce the sliding friction. For rough adjustments the carriage is released by a lever *a* and for fine adjustments the lever is locked and the adjustment made by a hand wheel, one revolution of which is approximately 0.004 inch. The standard reference scale *D* is fixed on the carriage at two points; one of these supporting points *d* is in the form of a pivot and the other *g* in the form of two opposing adjustable screws *f*, by which means the scale can be easily adjusted to the bed.

The reference scale consists of a 58 per cent. nickel steel bar, the middle portion of the upper face of which is highly polished throughout its length. This polished portion is engraved with division lines at every twentieth of an inch, the thickness of the lines being about 0.0002 inch. The coefficient of expansion of the scale is practically the same as that of tempered steel. The accuracy of the graduations over the 20" length is about 0.0001 inch to 0.0004 inch. The errors of graduation are given in a Certificate of Standardization supplied with each machine so that allowance can be made for them when making very fine measurements. The scale is protected by a brass casing, having a sectional cover, a portion of which can be removed to expose the parts of the scale on which readings are to be made.

An important feature in the design is the fact that the graduated surface of the scale is in line with the axes of the measuring faces, thus reducing to a minimum any errors of measurement which would arise from accidental deformation of the bed. If due allowance be made for errors in the standard, scale measurements in absolute value can be made with an accuracy of 0.00005".

The pressure indicator on the micrometer headstock (Fig. 2) enables measurements always to be made under the same conditions of pressure, which is approximately half a pound. It consists of a series of levers, one of which serves as an index pointer *J* moving over a scale *I* which is graduated with ten division lines on either side of a central mark, the division marks being about 1/20 inch apart; the movement of the pointer *J* from one division line to the next corresponds to a displacement of the measuring anvil of 0.00005". This also serves the purpose of comparing pieces against a standard piece in which it is easy to take readings to 1/5th of a division which is equal to 0.00001". This latter operation is the sole method of measurement on many instruments which are called Measuring Machines.

The machine can be used for internal measurements as well as external, as by simply giving a movable bush *n* a three-quarter turn the condition of the spring *m* operating on the measuring anvil of the headstock micrometer is changed from that of compression to tension. Special fittings are supplied for placing over the measuring faces to enable internal measurements to be made.

The machines now in course of construction have a micrometer screw with a pitch of 1/20 inch. The divided drum attached to this screw is graduated to 1/10,000 inch and readings to 1/100,000 inch can be made by means of the vernier *r*. The machine on demonstration, however, has a micrometer screw with a pitch of 1/50 inch and the divided wheel is graduated to 1/20,000 inch and readings to 1/200,000 inch can be made by means of the vernier.

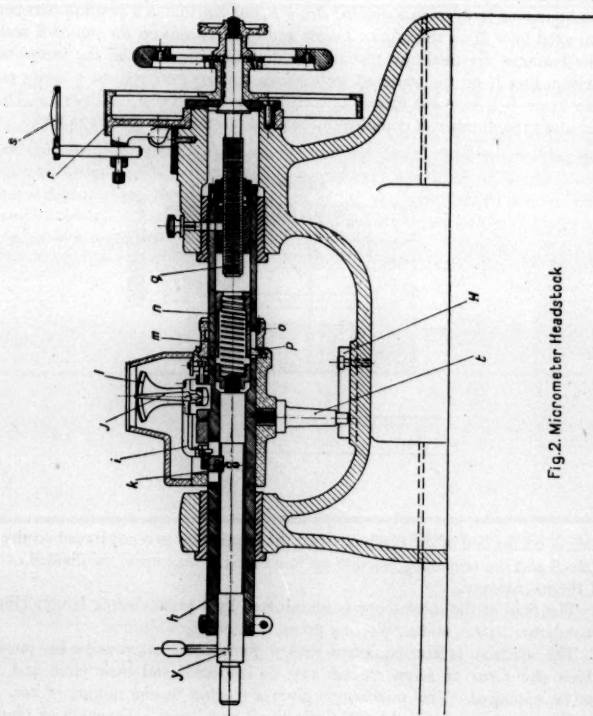


Fig.2. Micrometer Headstock

A micrometer microscope giving a magnification of 60 is used for reading the exact position of the standard scale. The approximate position of the sliding carriage is read on a scale attached to the bed which is divided in twentieths of an inch. The field of the microscope is as follows (Fig. 3):

A scale *B* of ten divisions on glass, each division being equal to 0.005 inch, two axial lines *D* engraved on the standard scale, two transverse hairlines *E* which can be moved by turning a divided drum *F*, one hairline in a position between the two axial lines *D* on the standard scale and the divisions on the standard scale *H*. The readings are made by turning the divided drum *F* until the image of the division line *H* on the standard scale which appears between the 0 and 5 on the glass scale *B* bisects the space between the two transverse hairlines *E*. To take a reading the number of complete inches and fractions up to 0.05 is noted on the

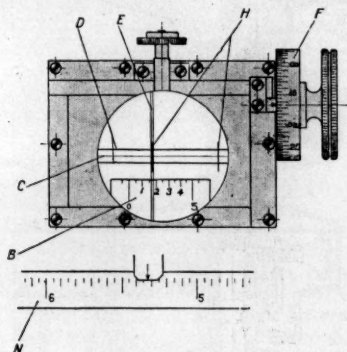


Fig. 3.

scale *N* on the bed of the machine, then the fraction up to 0.005 is read on the glass scale *B* and the remaining fraction up to 0.00005 is read upon the divided drum *F* of the microscope.

The field of the microscope is illuminated by a small electric lamp *s* (Fig. 1), a condenser lens *v*, and a reflecting prism *w* above the objective.

The machine is also equipped with a goniometric microscope by means of which the form of screw threads can be inspected and their pitch and angle can be measured. This microscope gives a reading to one minute of arc. It is possible also to measure the full "effective" and "core" diameters of plugs up to one inch in diameter. It is, however, far more accurate to make the latter measurements mechanically, for which in the case of "core" diameters special V pieces are supplied for inserting in the usual way between the measuring faces and the plug, and to measure the "effective" diameter needles suspended by threads from a support are used.

It is obvious from the foregoing that the S.I.P. Measuring Machine covers a very large range of work, for which a comprehensive set of fittings is supplied with the full equipment. We shall be pleased to give a full demonstration of this machine, or any of our other productions, at our showroom, 95 Queen Victoria Street, to those interested.

DISCUSSION

Prof. A. F. C. Pollard: I have no criticism to offer on this beautiful instrument, and am very glad that Mr Custance was able to bring it here this evening. Though such a machine may have little to do with optics, it should be of great interest to those troubled with interchangeable manufacture and also to those students of the Instrument Design Class whom I asked to be present. The unique feature of the machine is that the scale is placed *in* the axis of the measuring anvils and so is in that position where the errors due to imperfections of workmanship and setting are reduced to a negligible minimum.

REVIEW

Dictionary of British Scientific Instruments. Issued by the British Optical Instrument Manufacturers Association. (London: Constable and Co., Ltd.) Price 21s.

The main purpose of this volume is to provide a handy book of reference giving brief notes on the nature and uses of scientific instruments and also to indicate the makers. A useful section gives illustrations of over three hundred instruments. In addition, some forty-six pages at the commencement of the book are devoted to short descriptive articles on the British Optical Instrument Industry, Navigating Instruments, the Royal Meteorological Society, British Optical Glass, and the The Royal Observatory, Greenwich. Five half-tone plates give views of Greenwich Observatory and the National Physical Laboratory. We must confess that these additions appear to give an *embarras de richesse*. The book would have been cheaper and more dignified had they been entirely omitted, for although they are in themselves very interesting, the articles are superfluous in a dictionary.

The dictionary itself appears to be well arranged although not entirely free from errors; 'Leucoscope' is given as 'Lencoscope,' for example. We suggest that if a future edition of the work is issued references to fuller descriptions of the instruments should be added. This would make the book of much greater value from a scientific point of view. Also, instead of illustrations of the National Physical Laboratory, some account of the facilities given by that Institution for the testing of instruments would have been useful.

In spite of some defects, however, it should be of great service, especially to provincial workers and students who have not ready access to collections of instruments. It should be available in every reference library.

L. C. M.

ABSTRACTS

"The Annealing of Glass." By L. H. ADAMS and E. D. WILLIAMSON, *Journ. Franklin Inst.*, 190 (1920), 597, 835.

Quickly cooled glass is in an unstable condition due to stresses acquired in cooling. Such glass is said to be "strained," and if the strain is excessive, the glass may crack or even fly to pieces when handled, or when heated or cooled.

The paper discusses the manner in which temporary and permanent stresses arise in glass and it is shown that the process of annealing can best be carried out if the rate of release of the internal stresses is known for the various glasses and temperatures. Nine kinds of glass were tested and the release of stress at constant temperature was found to proceed usually according to the equation

$$\frac{1}{F} - \frac{1}{F_0} = At,$$

where F is the stress at any time t , F_0 is the initial stress, and A is a constant for the particular glass at a particular temperature, being a measure of the rate at which stresses are relieved. The variation of this rate with temperature follows the equation

$$\log A = M_1 - M_2 \theta,$$

where M_1 and M_2 are constants for a particular glass, and θ is the temperature.

At any temperature a glass requires a certain "annealing time." This is arbitrarily defined as the time required to reduce the stress (in optical units) from 50 to $2.5\mu\mu$ per cm. The 150° interval of temperature lying immediately below the temperature at which the annealing time is two minutes is defined as the "annealing range." At temperatures below the annealing range very little permanent stress can be introduced. The authors give directions for annealing various kinds of glass and it is shown that the best method requires that the glass be maintained at constant temperature (below the customary annealing point) for the appropriate time and then cooled at an increasing rate. A number of equations are given for calculating the stresses due to heating or cooling various shapes of glass specimens.

J. S. A.

"The Measurement of Graininess in Photographic Deposits." By LLOYD A. JONES and NOEL DEISCH, *Journ. Franklin Inst.*, 190 (1920), 657.

When a photographic deposit is examined under the microscope, it is found to be made up of a large number of minute particles or grains, consisting of metallic silver, which have been reduced from the silver halide by the action of the developer.

Measurements have shown that these grains vary in diameter from approximately .5 micron in slow plates up to from 1 to 10 microns in high-speed plates. The term "graininess" is applied to the lack of homogeneity due to aggregates of these grains composed of a relatively small number of the ultimate particles. The authors have developed a method of measuring such graininess. The fundamental assumption upon which it is based is that the graininess of a deposit is directly proportional to the distance at which the appearance of graininess becomes just imperceptible, provided that all other factors, upon which depends the ability of the eye to distinguish lack of homogeneity, are constant. For details of the apparatus employed reference should be made to the original paper. It is found that there is a very appreciable difference in graininess produced

by different developing agents. Graininess is not, therefore, altogether a function of grain size, as it has previously been shown that the size of the silver grain is almost independent of the composition of the reducer.

J. S. A.

"A Production of Panchromatic Sensitiveness without Dyes." By J. G. CAPSTAFF and E. R. BULLOCK, *Journ. Franklin Inst.*, 190 (1920), 871.

The authors describe the results of preliminary experiments which they have carried out in making films panchromatic by the use of a sodium bisulphite bath.

In one experiment films were bathed in 2 per cent. sodium bisulphite for ten minutes, the temperature being 70° F. They were then washed in running tap water (temperature about 60° F.) for times varying from 5 minutes to 30 hours, and subsequently allowed to dry spontaneously, hanging in a well ventilated dark room. Soaking in a large volume of water was always found to have the same effect as washing in running water, if the temperature was the same. The addition of alkali (*N/10* sodium carbonate) to the tap water reduced the time of washing necessary and increased the general sensitiveness. It was found later that even better results were produced by a .2 per cent. distilled water solution of pure potassium bicarbonate which was neutral to litmus and free from chlorides. The effect appears to be due to the sulphurous acid content of the sodium bisulphate solution. Small percentages of soluble bromides or chlorides diminish the effect.

J. S. A.